Author's response to reviews

Title: Agreement between parents and adolescents on emotional and behavioral problems and its associated factors among Chinese school adolescents: a cross-sectional study

Authors:

jiana wang (jiana0818@163.com)
li liu (liul@mail.cmu.edu.cn)
hui wu (wuhui@mail.cmu.edu.cn)
xiaoshi yang (yangxs@mail.cmu.edu.cn)
yang wang (allylove666@yahoo.com.cn)
lie wang (liewang@mail.cmu.edu.cn)

Version: 3 Date: 6 January 2014

Author's response to reviews: see over
Dear Prof. Raffy Mar Gella:

Thanks a lot for those comments on my paper (MS: 1510778420918727). I revised my paper as the reviewers advised and submitted the revised manuscript. Now, I would like to give a point-by-point response to the comments.

Answer to Reviewer’s comments:

**Reviewer: 1**

**Q1.** I appreciate the authors’ effort in adding more information to support their rationale of investigating specific factors associated with parent-adolescent disagreement. However, in several places, the argument is unclear and vague. For example:

(a). On p. 5, line 4-5, the authors wrote “…. a conflicting evidence existed regarding the direction of age effect on informant agreement.” This is unclear given that the study cited (Berg-Nielsen et al., 2003) showed that, with increasing age, discrepancies between CBCL and YSR decreased. I don’t see why this is a conflicting evidence. Please clarify.

(b). The sentence on p. 5, line 5-6, is vague. Please be specific as to how the quality of parent-children relationship could also “indirectly” influence the disagreement between parent and their children. Same issue for the statement in line 10-11 “different parental function itself can [directly] influence their child behavior and emotion.”

**Answer:**

(a) I felt very sorry for this mistake. I carefully checked this point and revised the mistake (page5, line6). We are very sorry for this point. If the change is not sufficient, we will be greatly appreciated for your further advices!

(b) I am sorry to make you confused and I changed my expression in the instruction sections (page5, line7,11). If the change is not sufficient, we will be greatly appreciated for your further advices!

**Q2.** I thank the authors for providing information regarding SES and class size in their response letter. It would be informative for the readers to know more about the sample characteristics. I suggest the authors add the info about SES and class size in
the manuscript.

**Answer:** Thanks a lot for this great advice. I have added the information about SES and class size in the method section (page7, line19; page8, line9-10). If you think it was not enough, I am looking forward to your further advice.

**Q3.** Maybe I am mistaken, but I don’t understand why Cronbach’s alphas for internalizing scale and externalizing scale are in ranges. I believe one Cronbach’s alpha for each internalizing and externalizing scales can be calculated using the scores after referencing age and gender norms.

**Answer:** I am sorry to make you confused. As far as I know, the scorings of the Chinese 1991 version of CBCL and YSR are divided according to age (<12/12) and gender according to Xin et al. 1992 investigating emotional and behavioural problems among 24013 students in 22 cities and standardizing the Chinese version of Achenbach’s scale. Therefore, I used the scoring to calculate Cronbach’s alphas. I eager to get the identical subscale scoring. I tried to consult related literatures and contact with Achenbach or other professors but failed. I look forward your further advice about this important question and I will really appreciate for your help!

**Q4.** I thank the authors for their responses to my comment regarding the single-item measurement (for parent-adolescent relationship and parental expectation). I understand their concern about the length of the questionnaire. Although internal consistency cannot be inferred using a single item, reliability can be assessed using the intraclass correlation (ICC), if the authors have data from more than one time point or if previous studies have used a similar item to support the reliability of such measurement (e.g., Korkeila et al., 2004; Wu et al., 2009, as the authors described?). Likewise, concurrent validity or predictive validity in relation to other measures can be examined. If the authors have information on these, please add it in the manuscript, as information like this would help the readers better evaluate the findings.

**Answer:** Thank you very much for your advice. I learned a lot from your advice. I don’t have data from more than one time point. Previous studies I described used a similar form with only one question but not a similar content and I can’t achieve data from their papers. I’m sorry but I can’t find other measures to be a criterion to
examine the validity. I will be greatly appreciated for your further advices!

Q5. The range of Cronbach’s alpha of the 7 subscales in the FES is 0.42-0.81. I wondered how many subscales’ alphas are lower than 0.70? Please clarify, as low internal consistency (e.g., under 0.70) may undermine the validity of the findings.

Answer: Thanks a lot for this question. Only two subscales’ alphas are lower than 0.70 and among the two, one subscale’ alpha is close to 0.7. Either the total score or subscales of FES have good reliabilities and validities. If you think it was not enough, please kindly let me know.

Q6. I agree with Reviewer 2’s suggestion about conducting a 2 x 2 repeated measure ANOVA with informant as a within-subject variable, and including age as variable in the analysis. It looks like the authors took the suggestion and conducted such analysis; however, there are several issues that await clarification:

(a) the description in p.11, line 2-3, did not accurately reflect this analysis.
(b) did the authors use age as a categorical variable in the analysis? If so, please describe how they categorize age and provide justification, if any.
(c) several significant 2-way and 3-way interactions were found, but no follow-up analyses were conducted to decipher the interaction effects. Post hoc comparisons are needed to break down the interactive effects of gender, informants, and/or age on the problem scores.

Answer: (a) Thanks a lot for this great advice. I have changed the expression to make it more accurate in the method section (page11, line6-8). If you think it was not enough, please kindly let me know.

(b) In order to make data suitable for the variance analyses, I used age as a categorical variable in the analysis. Age was divided into “11-14 years old” group and “15-18 years old” group. If you think it was not enough, please kindly let me know.

(c) Thanks a lot for this great advice. In our study, the effect sizes of interactions between the CBCL-YSR discrepancy scores and gender or age were all very small (< 1), and the t-test that was deleted also showed the very little effect of age and gender on CBCL and YSR. Therefore, I didn’t further use the Post hoc comparisons to break down the interactive effects. If you think it was not enough, I
will look forward to your further advice.

Q7. The authors conducted univariate analysis, as well as regression, to examine the association between parent-adolescent discrepancies and child, parent and family factors. Univariate analysis seems redundant to me, and it involves testing more than 30 models (with 3 outcome variables (DVs): total scale, internalizing and externalizing scales and 11 IVs), which should be justified with some sort of corrections for multiple comparisons.

Answer: I am sorry to make you confused. We used univariate analysis only as a preliminary screening. The univariate analysis was conducted respectively with 3 outcome variables. Each Pearson’s correlation coefficient represented only one relationship between two factors. I am sorry but I am not very clear whether need and how to do the corrections for multiple comparisons. If you want to give me some further advices, I will appreciate your help.

Q8. It is unclear if only one regression analysis (with all IVs) or multiple ones (with one IV at a time) were conducted. Please clarify.

Answer: I am sorry to make you confused. The regression analysis was respective and multiple ones (with one IV at a time) were conducted.

Q9. Line 19-21 on p.12 is unclear. Please revise.

Answer: I am sorry to make you confused. I have changed my expression in the result section (page13, line1-2). If you think it was not enough, I am looking forward to your further advice.

Q10. There are still many syntax and grammatical errors, as well as imprecise wording or improper usage of words. I encourage the authors to address these issues in their next revision.

Answer: I am sorry to make you confused due to my poor English language usage. I will carefully revise other problems about language usage at a later stage in the next revision process. I really appreciate your guidance and help for me.
Reviewer: 2

Q1. The authors now summarize on p. 4 some of the findings from the Rescorla et al. (2013) report of CBCL-YSR cross-informant agreement in 25 societies, which is an important addition to their literature review. As those 25 societies included China, I think the authors should report in the literature review that the mean r averaged across 17 scales was .46 for China, in addition to citing the range of correlations (.35-.60) reported in the Wang et al. (2005) for the same Chinese sample (plus 400 dyads excluded from the Rescorla et al. study due to selection criteria).

Answer: Thank you very much for your advice. I have added the information in the introduction section (page 4, line 13-17). If you think it was not enough, I will look forward to your further advice.

Q2. Although the authors mention on p. 4 that previous studies (including the 2013 Rescorla study) have reported higher problem scores for adolescent self-reports than parent reports in general population samples, they do not note that Rescorla et al. reported that YSR scores were higher than CBCL scores in all 25 societies. The informant effect sizes of 22%, 16%, and 18% for Total Problems, Internalizing, and Externalizing are much larger than those the authors report in Table 3. Therefore, on p. 14 in the Discussion when the authors comment on their YSR>CBCL finding, I think that they should cite the 2013 study and make it clear that their study replicated a finding already reported previously for 25 societies, including China. The Abstract is also misleading in this respect, because the authors state that China had a “different pattern” of agreement, followed by a sentence about adolescents reporting more problems than their parent. I would argue that the pattern of results obtained in this study is not different from that reported for the 25 societies in the Rescorla study, albeit the level of agreement is at the high end of those reported by Rescorla.

Answer: Thanks a lot for this great comment. I changed my expression in the discussion (page 15, line 6-13) and abstract sections (page 3, line 2). If you think it was not enough, please kindly let me know.

Q3. It is noteworthy that rs for internalizing and Externalizing were virtually identical in this study (both .61), also replicating the findings reported by Rescorla et al. (2013)
(.45 and .46) but contrary to the common assumption that agreement is better for problems that are more overt.

**Answer:** Thanks very much for this great comment. The similar rs indeed go against the common assumption. However, I found that many researches also reported a similar rs or even a higher rs for externalizing than internalizing. I’m sorry but I can’t give a reasonable explanation with my current knowledge. Maybe further systematic researches for these inconsistent and uncommon results are needed. If you want to give us more advice, we will really appreciate it.

**Q4.** In their response letter, the authors address my previous comment that they need to state why they used the 1991 version of the CBCL and YSR, even though their data were collected in 2010 and the 2001 versions of these instruments were available in Chinese at that time. However, some of the information they provide in the response letter is incorrect. The 1991 and 2011 versions of the CBCL and YSR have the same number of items (and only 6 items were replaced). Additionally, the Wang et al. study used the 1991 version early in their data collection process but the 2001 version later in their study. As noted in my previous review, the authors need to say something about this issue in their manuscript. I suggest on p. 8 they include something like the following statement: “The current study used the 1991 versions of the CBCL and YSR because the Chinese 2001 versions of the instruments (on which six 1991 items were replaced) were not yet widely available at the time the study was conducted. Therefore, the 1991 versions of the CBCL and YSR syndromes were used in the current study, which differ slightly from the 2001 syndromes in item composition and name.”

**Answer:** Thanks a lot for this comment. I have added some information in the method section (page 8, line 15-19) according to your suggestion. If you think it was not enough, I will look forward to your further advice.

**Q5.** As the authors have now conducted the 2 x 2 ANOVAs with informant as a within-subjects factor, reporting t-test results as well seems superfluous.

**Answer:** Thanks very much for this great advice. I have deleted the t-test results and relevant descriptions. If you think it was not enough, I am looking forward to your
further advice.

Q6. On p. 7, the authors imply that the purpose of the study is to help clinicians decide which instrument to use to screen for problems. Given that they state on p. 4 in the first paragraph that “both parents and adolescents are needed to obtain a comprehensive picture,” it seems odd three pages later to imply that only one instrument is needed. I suggest they revise the p. 7 sentence to state something like “providing evidence of the importance of obtaining reports from multiple informants.”

Answer: Thanks a lot for this great advice. I have changed the expression in the instruction sections (page7, line5). If you think it was not enough, I will look forward to your further advice.

Q7. On p. 8, I think the authors should state that both the CBCL and YSR were sent home to be completed (i.e., the YSR was not completed at school).

Answer: Thanks a lot for this comment. In our study, adolescents were asked to write the questionnaire excluding CBCL at school and parents completed CBCL at home. The informants were requested to complete the questionnaires without consulting each other aiming at ensuring that parent and self-reports were completed independently. If you think it was not enough, I will look forward to your further advice.

Q8. On p. 8 and elsewhere the names of the instruments (e.g., Child Behavior Checklist) should be capitalized, as should the names of the syndrome scales (e.g., Attention Problems). This makes them easier to spot in the text, which makes the results easier to follow.

Answer: Thank you very much for your advice. I have changed my expression according to your suggestion in my whole manuscript. If you think it was not enough, please kindly let me know.

Q9. On p. 9, the wording of the questions posed about parental expectations needs revision. Did the question state “How high are your parents’ expectations for your learning performance?”
Answer: Thank you very much for this question. I have changed my expression according to your suggestion in the method section (page10, line4). If the change is not sufficient, we will be greatly appreciated for your further advices!

Q10. On p. 11, I suggest the authors present the actual Cohen’s d values rather than the mean difference and SD. They should also indicate that the d values are quite small, based on Cohen (1988).

Answer: Thank you very much for this question. I have changed my expression according to your suggestion in the result (page12, line3-6) and discussion section (page15, line9-10). If the change is not sufficient, we will be greatly appreciated for your further advices!

Q11. On p. 12, line 9, I suggest the authors remind readers that they are reporting adolescent reports of their parents’ expectations. I also suggest they inform readers whether the FES means are low, high, or average, based on samples in the US or other countries.

Answer: Thank you very much for this great comment. I have changed my expression according to your suggestion in the result (page12, line11) and discussion section (page17, line6-9). If you want to give us more advice, we will really appreciate it.

Q12. On p. 14, the authors suggest that parents underestimate adolescents’ problems. However, as the Rescorla et al. (2913) study clearly indicates, the mean YSR>CBCL discrepancy, although a highly consistent finding across societies, fails to capture the fact that in many dyad CBCL scores are higher than YSR scores. The categorical agreement results reported for 25 societies clearly indicate that when “deviance” is analyzed (i.e., scores above a cutpoint), it is about equally common for parental reports of deviance to not be corroborated by adolescents as for adolescent reports of deviance to not be corroborated by parents. This is an important point that the authors should address on p. 14, if not earlier.

Answer: Thank you very much for this important question. I have added some descriptions in the discussion section (page15, line13-18) to make the expression more accurate. If you want to give us more advice, we will really appreciate it.

Q13. In their response letter, the authors make some interesting points about why
YSR-CBCL agreement might be higher in China than in Hong Kong. However, the mean rs were very similar in the Wang Beijing and Leung Hong Kong samples, as reported by Rescorla et al. (2013). Furthermore, one might expect the rapid economic changes in China in the past decade (plus more Western cultural influence) would presumably have served to make China more similar to Hong Kong rather than less so, suggesting that data for the current study, collected about a decade later than the Wang data, would have lower correlations, but in fact they are higher. I think the authors should address these cultural issues in a more nuanced way in their Discussion. For example, perhaps agreement in the Liaoning region of China is different from that in Beijing? Furthermore, among the largest rs in the Rescorla study were those for Denmark, clearly a Western country. The authors note in their response letter that Bilenberg’s Danish monograph included both clinical and general population samples, but the Danish sample in the Rescorla study only used the general population data. Therefore, the large rs for Denmark are not due to inclusion of clinical samples. Additionally, Algeria had quite large YSR-CBCL rs, as the authors note. The East vs. West dichotomy does not seem to fit well to characterize Algeria, which is clearly very different from China and had many centuries of French colonization. In short, I think the authors are making too simplistic an argument about Eastern vs. Western cultures and their manuscript would be improved by a more considered treatment of this important issue. The interesting data they report on family factors suggest one way they might approach this question. That is, whether within or between societies, in families with more closeness, cohesion, and communication, parent-adolescent agreement is likely to be higher.

Answer: Thank you very much for this great advice. It is certain that the rapid economic changes in China in the past decade would more or less westernize Chinese. However, Chinese traditional culture about 5000 years is deep-rooted. Westernization brings Chinese some modern ideas which might reduce the parent-adolescent generation gap and make them like friends. Meanwhile, Chinese keep traditional family pattern which make their members closer and more familiar with each others. Just like the description of Rescorla, the conventional dichotomies such as East/West
do not appear to explain the pattern of findings obtained in this research, but it is possible that familism might explain some of the differences. The economic level cannot simply indicate the parent-adolescent discrepancy. Therefore, we have reasons to believe that, as time goes on, the relationship of Chinese family might be better. However, the difference between provinces might also impact the results. So I have added some information about it in the discussion section (page14, line9-18). Furthermore, I have improved my manuscript focusing on family factors to better explain the issue you pointed out in the discussion section (page13, line15-17). More detailed information about family had been addressed in the next discussion section of ‘family environment’. If you think it was not enough, I am looking forward to your further advice.

These changes are made with considering reviewers’ comments. If the change is not sufficient, we are looking forward for further advices!
I will carefully proofread the whole manuscript and edit and re-word it about the whole problems and about language usage to improve the readability of my paper at a later stage in the revision process. I really appreciate your guidance and help for me.
Thanks in advance!

Best wishes,
Sincerely
Jiana Wang, Lie Wang